NOTES AND EXTRACTS.

THE MEASUREMENT OF WIND VELOCITY.

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Since the invention of the excellent instrument known as the Robinson anemometer, now so universally used by meteorologists for determining the wind movement, various scientists have endeavored to develop mathematically its theory, and to determine experimentally its constants, or the factors by which the wind movement is computed from the velocity of the cups. As a first approximation to the solution of what has proved to be a very difficult problem, the inventor concluded, after a study in which both theoretical consideration and experimental results were combined, that, in general, *the wind moved just three times as fast as the centre of the cups of the anemometer.* This relation has been widely accepted and long used as the true one, and was adopted by the Signal Service early in its history as the proper factor for its standard anemometer.

The instrument is so well known that a very brief description is sufficient; and the dimensions given are those of the Signal Service standard. A vertical spindle, very nearly a foot long, has, fastened to its upper end, a horizontal cross, consisting of four small steel arms, radiating from the centre and arranged at right-angles to each other. On the outer end of each arm is fixed a thin, metal, hemispherical cup, 4 inches in diameter; its centre being 6.72 inches from the centre of the cross, or axis of revolution. The circular rims of the hemispherical cups have their planes vertical, and the front or concave sides of the cups face in the same direction around the axis of rotation. The vertical spindle is supported in suitable bearings, and is provided at its lower end with an endless screw which gives motion to a train of wheels by which the revolutions of the spindle are registered. This anemometer (with a factor 3) is presumed to make 500 revolutions of its cups for each mile of wind movement, and the dials are graduated and numbered in accordance with this supposition.

Mathematical analysis has failed to develop in useful form the law for the anemometer, and the results of experiments, as made by various investigators, disagree with each other to such an extent that it is impossible to tell which are correct. This is doubtless due to the unfavorable conditions, in many respects, under which the experiments are necessarily made, and the imperfect and unequal elimination of the effects of various disturbing circumstances to which all experiments are subject.

The method of experiment that has been attended with the greatest measure of success has been to whirl the anemometer through still air at a known velocity and measure the velocity of its cups, from which, when the experiment is repeated at several widely different velocities, the law of the anemometer is worked out. In order to whirl the anemometer conveniently, it is placed on the end of a long horizontal arm that is properly balanced upon, and adapted to revolve about, a vertical axis. The use of such whirling arms is always attended with certain complications, in consequence of which the results obtained are known to be more or less in error, and the amount of this error is very uncertain and difficult to determine. It is quite a necessity that all these experiments be made in a closed room; though in several cases investigations have been carried on in the open air, the most recent of which are those made in England by the wind-force committee of the Royal Meteoorlogical Society. Under such circumstances the problem becomes still more complicated, as the natural wind blowing past the anemometer is rarely or never so small that its effect can be ignored, nor can it be readily determined. In this latter respect, it can be shown that, leaving other things out of account, the effect of a natural uniform wind is to make the cups go faster than they otherwise would. A uniform wind, however, in nature is an anomaly, so that the error for this condition is quite beyond accurate determination. In fact, the results of all open air experiments on whirling machines can be considered as only coarsely approximate.

The largest machines heretofore used in closed rooms have had arms less than 14 feet from the centre to the axis of the anemometer. As the various unavoidable errors are, in nearly all cases, larger in proportion as the whirling arm is shorter, the best results are to be expected by the use of very long arms.

Finding it very unsatisfactory to attempt to determine the anemometer constants from the experiments already made, the Chief Signal Officer directed a new investigation to be made, in which it was endeavored to profit by the experience of all previously engaged in such work, and before passing to a description of the details of the methods used in these experiments, or the results, it will be well to consider some of the more important difficulties and sources of error to be encountered.

The anemometer on the whirling machine operates under a variety of conditions that are essentially different from its circumstances in the open air:

(1.) On the whirler it is itself in motion in stationary air, while when in use the anemometer is stationary in moving air. We cannot see why there should be any difference in the results for these two conditions, and, in the absence of experimental evidence to the contrary, we are rather forced to make the seemingly natural assumption that there is no sensible difference.

(2.) On the whirler the anemometer is constrained to move in a circular path, and the tendency to fly off from this path is accompanied by increased pressure of the spindle in its bearings, the result of which is increased friction and possibly slower motion of the cups. When it is considered that, with a whirling arm 14 feet long and the weight of the revolving parts of the anemometer about 1 to 2 pounds, the pressure under which the spindle revolves is seven or eight times the normal pressure in the open air, it seems quite probable that the whirling-machine experiments should be affected in a sensible degree from this extra friction. I am not aware, however, that experiments have been made to determine this error, it being, in general, passed over with little or no remark.

(3.) Another effect attending the circular motion on the whirling arm arises from the fact that when the spindle of the anemometer is vertical those cups that are nearer the axis of the whirling arm travel at a less velocity and experience less wind than those on the opposite side of the anemometer axis. Now, since the whirling arm can be made to revolve in two directions, while any particular set of anemometer cups will always revolve in the same direction, depending on the manner in which the cups are set upon the cross-arms, and how the latter are attached to the spindle, it results that when the whirler is revolved in one direction the cups that experience the greater velocity will have their concave surfaces presented to the wind, while those that are opposite experience a less velocity and present their convex surfaces to the wind. In such a case the cups will run faster than for normal conditions of equally distributed wind, or, in other words, for motion in a straight line. The case is exactly reversed when the motion of the whirling arm is reversed, and under these conditions the anemometer runs slower than its normal rate, or its rate for straight-line motion. It is considered that the mean of results obtained for the two directions of rotation of the whirling arm gives the value that would have been found had the motion of the anemometer axis been along a straight line instead of a circle. No experiment, therefore, in which the axis of the anemometer is perpendicular to the plane of revolution of the whirling arm, is complete, unless the results are taken for rotating the arm in opposite directions. No attention seems to have been paid to this consideration in the experiments so recently made in England, and all the results (the Robinson factor) are therefore either too high or too low, according to whether the anemometer and whirling arm revolved in the same or in opposite directions, and by an amount depending, in the main, upon the relative lengths of the arms of the anemometer and that of the whirler.

(4.) Still another peculiar effect results from the circular motion of the anemometer, namely: the number of revolutions of the cups, as observed registered by the counting mechanism after a given interval, is not the actual number of turns, but only the number in relation to the whirling arm, which itself may in the meantime have made several revolutions. To find the real number of turns during any time, it is necessary to add or subtract from the apparent number one turn for each revolution made by the whirling arm. This is to be added when the arm and the anemometer revolve in the same direction and to be subtracted for the reverse condition. A knowledge of this fact has led some to suppose that such a correction should be applied to all whirling-machine experiments, but such is not the case, for the reason that, while such a method would give the actual number of revolutions of the anemometer cups in relation to any fixed object, as the floor of the room, for instance, yet, since the direction in which the air acts upon the cups of the anemometer is itself as the arm revolves, continually changing, relative to the fixed object, it follows that the real number of turns of the cups in relation to the air which makes them revolve is, after all, simply the apparent number of turns indicated by the dials. It is therefore erroneous to apply any correction of this kind as one is at first led to suppose.

(5.) The last to be considered as, perhaps, the most serious, and undoubtedly the most difficult to dispose of, is what has been called the "mit-wind." When the arm, including its accessories, is in motion there is a tendency to drag the air around with it, and this movement of the air with the arm prevents the anemometer from experiencing the full quota of wind effect corresponding to the real motion of the arm. Before any correct results can be obtained it is necessary, therefore, to be able to measure to just what extent the air, through which the anemometer passes, fails to remain stationary. The above conception of "mit-wind" is possibly somewhat different from that entertained by others who generally make use of the term when referring to a more or less continuous movement of rotation imparted to the whole mass of air through which the arm revolves. Such a condition undoubtedly exists in no small degree where arms of moderate length are used, especially if they are somewhat stoutly built and expose considerable surface to the air. Even with long arms (30 to 40 feet) the rotation of the air, as a whole, is quite perceptible. In any case it is necessary to know, as previously stated, to just what extent the air, through which the arm passes, fails to be in a stationary condition when met by the anemometer, and hereafter the word "mit-wind" will be used to refer to any motion which the air may have in consequence of the action upon it of the whirling arm and accessories.

The measurement of the "mit-wind" has generally been the most unsatisfactory feature of all experimental investigations. Very delicate and sensitive air-current meters have been set up around the whirling machines and near the path of the anemometer, by which means it was intended to measure the slow rotation of the air, but these meters are so strongly affected by the violent commotion and disturbance that immediately follow the passage of the anemometer that the much slower movement the air may have just before it is met by the anemometer is quite lost, and more or less misleading results only are obtained.

After the above in relation to the whirling machine, and its sources of error, a few remarks upon the anemometer, especially its general law of motion, will not be inappropriate.

Robinson, the inventor, was at first satisfied, that, after neglecting friction, the factor 3 would very closely represent for all velocities how many times faster the wind moved than the cups. Mature consideration will show that neither 3 nor any other single factor can represent, with reasonable accuracy, the relation between the velocities of the wind and that of the centres of the cups, unless the range of velocities is made very small. Since it is impossible to make an anemometer without friction, and since from the peculiar construction of the instrument the wind acts to produce rotation on only a part of its cups at a time, the action on the remaining cups being in a direction to *prevent* the above motion, it results that the difference only of the wind pressures is effective in causing rotation, and when the wind is very light this difference in its pressures is insufficient to overcome the friction in the moving parts. It is evident that for this case in which the cups actually remain stationary the velocity of the wind is infinitely greater than that of the cups. In many anemometers a wind velocity of a mile an hour or more is necessary to start the cups, and when they do fairly move in a light wind it will be so slowly that a factor like 3 is not nearly large enough. The faster the wind blows the smaller the factor becomes, so that for high winds the factor 3 is too large. It is quite impracticable, therefore, to represent at all accurately the anemometer law by any single factor, and the next simplest thing that it has been found could be done is to determine, as it were, how fast the wind must blow to just start the cups. This being determined we can next find a single factor such that if we multiply the velocity of the cups by it and then add to the result the velocity necessary to just start the cups, the two together will give a quantity that will, in many cases depending upon the dimensions of the anemometer, quite closely represent the actual wind movement through a considerable range of velocities. A still closer agreement can be obtained by introducing a third term in which the square of the velocity of the cups is multiplied by still another factor.

The experiments then, such as have been indicated, are made in order to furnish data from which factors, like those suggested above, can be computed.

The discussion already given of the general law of the anemometer and the sources of error in the whirling machine experiments, will serve to point out various special characteristics that will tend to lead to the most accurate or satisfactory results, thus:

1st. The anemometer must have the smallest amount of friction possible, and its friction must be the same one day that it is another.

2d. The whirling arm must be as long as possible, not only because the effect due to the centrifugal tendency is less, but also because the motion of the anemometer in a very large circle comes more nearly being motion in a straight line, and, moreover, the tendency of a long arm to set the surrounding air in motion, or to generate "mit-wind" is less.

3d. The arm must be made as slender as possible, as this also will favor the development of only a little "mit-wind."

With these general characteristics in view, an anemometer spindle was constructed, the upper end of which was provided with a bearing consisting of a peculiar set of rollers, by which the friction was reduced to a minimum, and but small under the influence of the centrifugal tendency.

The arm of the whirling machine was made of the ordinary wrought-iron pipe, 2¾ inches in diameter at the centre and 1½ at the outer end. The length of the arm from the centre to the point at which the axis of the anemometer was carried was 35 feet, but the construction was such that 7 feet could be taken off the outer end, leaving the arm 28 feet long, experiments being made with both lengths. Galvanized-iron telegraph wires of ordinary size (No. 9) were, by means of bolts, drawn very tight over struts at the axis of the arm, which was thus, though very slender itself, made very rigid, and, at the same time, exposed a minimum amount of surface to the air. A short end of the arm opposite the long one carried a heavy counterpoise of iron weights, by which the whole was nicely balanced upon the vertical axis, which turned with great smoothness and freedom in suitable bearings; the height of the arm above the floor was about 8 feet, and the anemometer cups 2 feet higher.

Motion was given by hand-power applied to a horizontal hand-wheel, something like a pilot-wheel, but with the pins set on the side instead of the edge of the wheel. The first intention was to revolve the arm by means of cranks and gearing, but the latter were found to work, even at the best, with sufficient jar to impart a tremulous motion to the slender arm, and were rejected.

The velocity of the arm, as also that of any of the anemometers that might be placed on its outer end, was automatically recorded on a sheet of paper by means of a chronograph, the time being determined from a carefully adjusted seconds-pendulum, which was arranged to momentarily break an electric circuit every swing. In addition to this permanent record, the motion of the arm was, by an ingenious arrangement of electrical devices, made to give an audible indication of its velocity, so that the operator could tell, almost instantly, and with great precision, whether the velocity was higher or lower than the rate desired and so control the driving power as to maintain practically a perfectly uniform motion of the whirler. The rate was not only thus preserved uniform, but, by a slight and conveniently-made change in the devices, they served to regulate the velocity at any of the rates, so that the end of the arm, or more properly the axis of the anemometer, was moved at 2½, 5, 10, 15, 20, and 30 miles per hour.

Through the courtesy of Gen. J. C. Black, Commissioner of Pensions, this large whirler was set up in the west portion of the great court of the Pension Building. The space, which is roofed over and otherwise closed on all sides, is nearly 90 feet square and very high, so that it offers extremely favorable conditions for these experiments.

In order to be able to observe whether the air in the court was perfectly still, as also to note the amount of disturbance set up by the rotation of the arm, several small tissue-paper streamers, suspended by fine threads from cords stretched across the court, were arranged at various points so as to hang a little above the path of the cups of the anemometer. These are very sensitive to slight air currents and proved very useful throughout the experiments.

Many efforts were made to measure the "mit-wind" effect by use of stationary current meters, but without the least success. The following method was finally adopted with most satisfactory results:

A very small and delicate anemometer was constructed, having cups made of small paper cones. These were 1¼ inches in diameter at the base, the slant height of the cone being the same, so that the angle at the apex was 60°. The arms to which these cups were attached were 1.47 inches long to the centres of the cups; the weight of these arms and cups was a little less than 2½ gramms, or about half the weight of a nickle 5-cent piece. The spindle of the anemometer was correspondingly delicate, and gave motion to a single wheel which was arranged to break an electric circuit once in each revolution, corresponding to 191 revolutions of the spindle. The part which formed the immediate support for the spindle and wheel work was scarcely as thick as a lead pencil.

Before any use could be made of this anemometer it was necessary to determine its constants. This was done by whirling it upon the machine, but in order to remove it, as far as possible, from any influence of "mit-wind" or other disturbance in the vicinity of the arm, a slender support nearly 10 feet high was erected at the end of the arm and the little anemometer placed upon the upper end of this. Being so small itself as to produce practically no appreciable "mit-wind," and being out of reach of the disturbance due to the arm, it was possible to whirl this anemometer in perfectly still air, especially since, owing to the extreme lightness of its cups, in consequence of which they would almost instantly take up their proper velocity, it was rarely necessary to prolong the motion of the arm for more than 1 to 2 minutes, except for very low velocities. The constants of this anemometer were thus determined repeatedly during the progress of the work.

When using this anemometer while experimenting upon larger ones it was carried just a little below the level of the large anemometer and about 6 feet in front of it, as shown in the illustration. From the record on the chronograph sheet for the small anemometer, it is possible, using the constants previously determined, to calculate just how fast the small anemometer passed through the air, as shown by the motion of its own cups. If there is any "mit-wind" it is at once shown in this way, as the velocity determined from that of the cups will be less than that of the end of the arm, as was the case in every instance, except when experiments were made at certain times during which the outer doors and windows of the court were open, permitting air currents of noticeable velocity to circulate about the court. Experiments were only made at such times to show what would result, and the values obtained were not used in computing the final equations.

In addition to the experiments made with the standard Signal Service anemometer, other instruments in which the cups and arms were of various relative dimensions or of modified form were tested, the results of which will be published later.

Two sets of constants were computed for the Signal Service anemometer and are given below, the first set forms the terms of an equation of a straight line, the second, those of a quadratic equation:

$$V = 0.225 + 3.148v - .0362v^2$$
$$V = 0.994 + 2.739v$$

The degree of closeness with which the true wind velocities can be computed by these equations is indicated in the table below, in which also appear the velocities computed by the old factor, 3:

Wind velocity, miles per hour.	Old factor, 3, gives velocity—	Straight line equation gives velocity—	Quadratic equation gives velocity—
3	14 per cent. too small.	13 per cent. too great.	3 per cent. too small.
5	8 per cent. too small.	5 per cent. too great.	0.3 per cent. too small.
10	2 per cent. too small.	O. K.	2 per cent. too great.
15	O. K.	2 per cent. too small.	0.1 per cent. too great.
20	2 per cent. too great.	2 per cent. too small.	0.1 per cent. too small.
25	5 per cent. too great.	O. K.	0.2 per cent. too small.
30	8 per cent. too great.	2 per cent. too great.	0.2 per cent. too great.
*40	15 per cent. too great.	5 per cent. too great.	±.

* This value was computed by means of the quadratic equation.

These constants, it must be remembered, apply to the case of the anemometer being moved forward through still air and at an almost perfectly uniform velocity. In the open air, in addition to the fact that the anemometer is itself stationary, the motion of the air is far from uniform, but is constantly and very suddenly changing, frequently from quite low to quite high velocities No account seems to have been taken in previous experiments of the questions that arise in this connection, notwithstanding their great importance. The weight of the anemometer cups, of the ordinary size, even when made as light as possible, is sufficient to cause them to continue in motion for a considerable time after a wind which may have started them has ceased. It is argued that for any amount gained in this way a corresponding amount is lost when the wind begins to blow, as at this time the weight of the cups makes them lag behind, but while some compensation is effected in this way, it can be demonstrated that heavy cups in a variable wind run faster then they would in a uniform wind of velocity equal to that of the mean of the variable wind. In recent experiments this gain has amounted to an excess of nearly 10 per cent. in the mean velocity, and even more than 30 per cent. in single instances. By variable velocity in this connection is meant, not a slow change from hour to hour, but the sudden and somewhat violent fluctuations that occur within a minute or even less and are so noticeable on any somewhat windy day.

The results of the open air comparisons are not sufficiently complete, as yet, to warrant further discussion, but it is very evident that the precise action of an ordinary anemometer in a variable wind cannot be completely worked out by whirling machine experiments at uniform velocities. Experiments are now in progress in which the weight of the cups is varied without changing them materially in other respects, and it is anticipated that much valuable information is to be gained in this way.

The general appearance of the apparatus and accessories is shown in the accompanying illustration, which is taken from a photograph made during the progress of the work. The arm at this time was 35 feet long.

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